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Market Structure and Trader Anonymity: An Analysis of Insider Trading

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Abstract

This paper examines the degree of anonymity—the extent to which a trader is recognized as informed—on alternative market structures. We find evidence that is consistent with less anonymity on the NYSE specialist system compared to the NASDAQ dealer system. Specifically, when corporate insiders trade medium-sized quantities (500–9,999 shares inclusive), NYSE listed stocks exhibit larger changes in proportional effective spreads than NASDAQ stocks. Taken together, these findings are consistent with Barclay and Warner's (1993) contention that stealth (medium-sized) trades are more likely based on private information and insider trades are more transparent on the NYSE specialist system relative to the NASDAQ dealer system. The results support the hypothesis by Benveniste, Marcus, and Wilhelm (1992) that the unique relationship between specialists and floor brokers on the NYSE leads to less anonymity.

I. Introduction

The past decade has seen a proliferation of new markets and different trading systems. This growth has also generated a vigorous debate as to how markets should be organized and regulated. Among the important issues in market design and performance is the issue of trader anonymity—the extent to which a trader is recognized as informed. Trader anonymity may affect market liquidity, volatility, and informational efficiency. In addition, important regulatory issues regarding front running and dual trading are related to the degree of anonymity. Finally, the advent of electronic screen trading systems raises the issue of whether markets perform better with traders' identities being revealed or concealed, or whether

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there is an optimal level of anonymity that improves the welfare of the market's participants.

Despite the importance of this issue, many models of trading and market microstructure simply assume that markets are anonymous or that trader type is unobservable, and that market makers who observe only order flow set prices.[1] While this assumption describes many trading systems including NASDAQ and other electronic systems, it does not characterize every trading venue. For example, in the NYSE specialist system nearly 65% of the trading volume is channeled through the floor brokers acting as agents for the public customers.[2] This aspect of trading on the NYSE may have important implications for the price discovery process.

Benveniste, Marcus, and Wilhelm (BMW) (1992) note that floor brokers deal repeatedly with the same specialist and are easily identified. They in turn argue that these repeat dealings will engender truth telling by the floor broker when the trader is believed to be informed. In particular, brokers who inform the specialist that the desired trade is possibly informed will receive ex post benefits in their dealings with the specialist. These benefits could include a willingness by the specialist to fill the remainder of broker orders that are only partially filled through limit orders. Moreover, brokers who do not inform the specialist of information trades will experience ex post sanctions from the specialist. One obvious sanction is to not fill remainders of orders executed against the limit order book. Furthermore, the specialist can provide less attractive price schedules to brokers with reputations for trading on private information. In other words, there appear to be mechanisms by which the specialist can elicit some information about traders' motives from floor brokers. Thus, specialists will likely behave differently when informed trades arrive than when trades are uninformed. By contrast, BMW (1992) argue that NASDAQ dealers will not behave differently on informed and uninformed trades since they are unlikely to have information regarding trader type.

In this study, we test the hypothesis that there is less anonymity on the NYSE than on NASDAQ by comparing the impact of insider trading on market maker behavior for the two markets. We assume that corporate insiders are better informed than outsiders and therefore trades by insiders are more likely to be motivated by private information.[3] Moreover, we focus on stealth or medium-sized trades by insiders, as these are most likely to be information motivated, according to Barclay

[1]For example, models based on asymmetric information by Glosten and Milgrom (1985), Kyle (1985), Easley and O'Hara (1987), and Admati and Pfleiderer (1988) assume that the identity of the trader submitting the order is not known. Notable exceptions are the models that allow for sunshine trading, i.e., Admati and Pfleiderer (1991), the model by Forster and George (1992), which allows for some transparency of the future direction and magnitude of the uninformed or liquidity trades, and the models by Roell (1990) and Fishman and Longstaff (1992) that analyze dual trading's effect on market behavior.

[2]Approximately 85% of all orders in the NYSE are submitted through the SuperDot or computerized order execution system. However, the volume of SuperDot orders is only about 35% of the total volume that is traded. Thus, orders entered through the SuperDot system are the small trades while the large volume trades are handled by the floor brokers.

[3]Insiders could also trade for liquidity reasons. Therefore, the information that a specialist elicits from the broker will only provide a noisy signal of the insider's motive.

and Warner (1993).[4] If NYSE specialists can elicit information from floor brokers regarding the type of trader, then active specialists will protect themselves more on medium-sized insider trades than on non-insider trades. On the other hand, NASDAQ as an electronic dealer market is likely to be more anonymous than the NYSE. Thus, we would expect NASDAQ dealers to behave passively and not alter their behavior on medium insider trades. On net, we should observe a positive difference between specialist and dealer responses to insider trading.

While this is the first study to explicitly test whether markets are differentially anonymous, there have been previous studies of the effects of private information on market maker behavior.[5] Our approach differs from typical studies in two important ways. First, prior empirical work generally focuses on the price or quote response to information conveyed by all trading activity in a stock during a particular period. By contrast, we focus our attention on the effects of insider trading on market maker behavior. Numerous authors (see, for example, Seyhun (1986)) have documented that registered insiders earn significant profits on their trades, consistent with the notion that they are informed. Second, our focus on the effect of insider trading on market maker behavior allows us to examine the difference between their behavior on insider trading and non-insider trading days.[6] By using non-insider trading days for the same stock as a control sample, we can effectively control for other costs of market making such as inventory and order processing costs, which typically impact (effective) spreads. Many prior studies control for these factors by estimating the fixed cost components of market making and the effects of prior trades and quotes on market maker behavior. Notable exceptions include Huang and Stoll (1996) who use a matched firm approach (and examine the difference in execution costs between the matched firms) and Lee, Mucklow, and Ready (1993) who examine changes in transaction costs for the same firm around earnings events.

Our study builds on the Huang and Stoll (1996) and Lee, Mucklow, and Ready (1993) approaches. Our use of the difference between execution costs for the same firm mirrors Lee, Mucklow, and Ready's work. However, we also draw upon Huang and Stoll's matched firm approach. Specifically, we look for differences in reactions to insider trading between New York and NASDAQ firms that are similar in terms of ex ante characteristics. This approach is designed to control for the effects of characteristics other than trader transparency on market makers' reactions to insider trading. Our match variables include proxies for firm size, risk, stock price, and typical insider trading activity.

A few recent studies have focused upon specific sets of trading activities and their effects on market quality. Meulbroek (1992) and Cornell and Sirri (1992), as well as Chakravarty and McConnell (1999), investigate illegal insider trading

[4] A natural question is why insiders in NYSE listed stocks would employ stealth trades if they tend to be treated as more likely information motivated. We conjecture, as do Barclay and Warner, that the costs of executing several smaller trades are sufficiently high to discourage their use, while large trades must often be conducted "upstairs" where anonymity may be more compromised.

[5] See, for example, studies by Glosten and Harris (1988), Hasbrouck (1988), (1991a), (1991b), Hausman, Lo, and MacKinlay (1992), Foster and Viswanathan (1993), and Madhavan, Richardson, and Roomans (1997).

[6] Non-insider trading days are based on five-day control periods that begin 11 days after the analyzed insider trade.

activity. Their results are somewhat conflicting. Meulbroek analyzes the trades of individuals later prosecuted by the SEC, and finds that the market impounds the information inherent in these activities into stock prices. Cornell and Sirri (1992) find that prosecuted insider trading in Campbell Taggart stock was associated with increases in volume, which apparently offset the market maker's incentive to widen the spread. Finally, Chakravarty and McConnell (1999) find that Ivan Boesky's trades in Carnation's stock prior to its acquisition had no discernibly different effect on prices than uninformed trades.

Not only do the above studies provide conflicting evidence concerning the effect of insider trading on market quality, but their results may not be generalizable, given their focus on prosecuted trades. Chakravarty (2001) avoids this problem by focusing on identified individual vs. institutional trades and their impact on market quality. He is able to do so by using the TORQ dataset, which provides such distinctions concerning order originators. Unfortunately, the use of TORQ data eliminates Chakravarty's ability to discuss the relative anonymity inherent in the NYSE vs. NASDAQ systems, as the TORQ sample is comprised entirely of NYSE stocks.[7] Our paper's contribution is to highlight differences in the reaction to insiders' trading across these two markets.

Our primary measurement variable is the difference between effective spreads on insider trading and non-insider trading days. This change in effective spread due to insider trading is treated as a proxy for the change in the market maker's adverse selection component. In other words, we assume that either the inventory and order processing components do not change between our control period and insider trading day or that our matching procedure handles the predictable changes in these components. Changes in effective spreads should be larger when market makers are more certain that they are trading against an informed individual. Therefore, larger effective spread changes in a particular trading venue are consistent with differential anonymity.

We document a statistically and economically significant difference between the two markets' average responses to insider trading—the change in effective spreads due to insider trading is larger for the NYSE sample than for the NASDAQ sample. Our evidence is consistent with the hypothesis that there is less anonymity on the NYSE than on the NASDAQ.

The remainder of this paper is organized as follows. Section II describes our empirical design. Section III presents our data. Our primary results on different levels of anonymity across markets are presented in Section IV. Section V concludes.

II. Empirical Design

We measure changes in market maker behavior due to insider trading as the difference between the average effective spread on the insider trading day and a corresponding average daily effective spread measure during a control period. We focus on daily effective spread measures for several reasons. First, we do not know the precise time at which the insider trade took place (only the day).

[7] Moreover, TORQ data is based on orders submitted through the NYSE's SuperDot system, which eliminates the role of the floor broker.

Second, if the market maker is not completely certain of the information content of a detected insider trade, then the inside information may not be completely incorporated into the stock price on the insider's trade, and the market maker will continue to face some adverse selection risk and thus choose to protect himself throughout the day.[8,9]

We choose to focus on effective spreads instead of quoted spreads primarily because of the nature of the market maker's potential responses to an insider trade.[10] In general, the market maker has the option to change either side of his quote and/or transact at a price that is within the quotes.[11] If the market maker is relatively certain of the information content of the trade, he may choose to alter either or both sides of the quotes.[12] Alternatively, if the market maker is uncertain about the information content of the insider trade, he may choose to transact at the bid or ask, as opposed to inside the quotes. Either of these responses will be picked up by our measure of effective spreads (see equation (1)), but only the former response would be measured if we analyze quoted spreads. Another reason we analyze effective spreads is because Petersen and Fialkowski (1994) show that quoted spread is no longer an accurate measure of transactions costs when trades are executed inside the spread (50% of the time in their retail order sample). They also document that when the posted spread widens, only 10% to 22% of the increase appears in the effective spread. Finally, the quoted spread is only valid for the quoted depth or size and, therefore, it captures the cost of transacting relatively small volumes.

For each transaction, we calculate a proportional effective spread as twice the difference between the midpoint of the standing bid-ask quote and the trade price, as a proportion of the standing bid-ask quote midpoint. This is then averaged across all the transactions on any particular day. Let n_t be the number of transactions for day t relative to the insider trading day, which is denoted as day $t=0$. Let P^A_{it-1} and P^B_{it-1} be the ask and bid quotes, which are at least five seconds before any transaction i at a price of P^T_{it} on trading day t. Then the proportional effective spread on day t is

$$(1) \qquad \mathrm{PES}_t = \frac{1}{n_t}\sum_{i=1}^{n_t} \frac{2 * \left|P^T_{it} - \left[P^A_{it-1} + P^B_{it-1}\right]/2\right|}{\left[P^A_{it-1} + P^B_{it-1}\right]/2}.$$

[8]Importantly, Chakravarty (2001) shows that post-trade measures of transactions costs are higher for institutional trades, consistent with exactly this sort of market maker behavior.

[9]Ancillary tests for differential market maker behavior on other days around the insider trading day yield insignificant results.

[10]Table 7 presents an analysis of trader anonymity using quoted spreads, but it is included primarily for comparison purposes.

[11]This action is subject to institutional constraints. If the trade is for less than the quoted depth, then the specialist can only change his quoted prices after the current transaction.

[12]However, BMW (1992) argue that such reliable information is unlikely to be received by the market maker (p. 75). Instead, BMW argue that any information regarding trader identity is more likely to be received by the NYSE specialist than by the NASDAQ dealer; not that traders on the NYSE are perfectly transparent. Moreover, adjusting the quotes leaves market makers open to greater competition from prevailing quotes by other market makers, and/or in the extreme, forces them to transact at an unfavorable price on small future trades.

We also calculate time-weighted daily proportional effective spreads, which we denote PESTW. The time weighting controls for the percentage of the trading day that the calculated PES is current (i.e., until the next PES is calculated).

Our control period average PES is the average of PES_t over days $[t+11, t+15]$. The advantage to using days $[t+11, t+15]$ is seen if insider trading likely follows information events. Specifically, if insiders typically trade after an information event, the event likely affects market maker behavior[13] and a more appropriate benchmark would take into account this new regime. Thus, a control period estimate of market maker behavior should also be after the event, and this is ensured if we estimate the control period over $[t+11, t+15]$. Indeed, our data suggest a strong tendency of insiders to trade after information events. Restricting usable observations to those with no information events between the control period and insider trading day (inclusive), eliminates far more data points when the control period is $[t-15, t-11]$ than when it is $[t+11, t+15]$. Formal evidence documenting insiders' preference to trade after a particular corporate event (earnings announcements) is found in Garfinkel (1997).

Our insider trading day PES is the PES on day $t = 0$. Note that PES_t is non-zero anytime the transaction price differs from the quote midpoint. Thus, a market maker's reaction to a potentially informed trade, perhaps an unwillingness to provide liquidity to incoming market orders, could cause fewer orders to be executed inside the quotes, leading to an increase in the effective spread. This measure of effective spread (or response to trading activity) implicitly assumes that the normal liquidity premium is a constant fraction of price.

A. Controls

Our tests examine changes in trading costs (the difference between effective spreads on insider trading days and non-insider trading days) as a function of the stock's listing exchange. However, firms listed on the NYSE likely differ systematically from NASDAQ firms in terms of their innate characteristics. To the extent that these characteristics affect the market maker's reaction to insider trading, we must control for them. We approach this problem by choosing pairs of firm insider trading days (one each from NYSE and NASDAQ) that are similar in terms of their firm size, ex ante risk, ex ante stock price, and typical insider trading behavior, and then examining the difference between the two days' market maker reactions. We describe our proxy variables for each of these match variables and the reason for their choice below.

Factors besides insider trading, such as news announcements, have also been shown to significantly affect trading costs.[14] Therefore, we eliminate observations with firm-specific earnings or distribution news in and around the analysis window. Details are provided in the data section.

Unfortunately, given our large initial sample size, we cannot easily identify all news announcements. Therefore, we also exclude from our analysis, those observations where the control period and insider trading day appear very differ-

[13]See, for example, Kim and Verrecchia (1991).

[14]See Lee, Mucklow, and Ready (1993) and Maddala and Nimalendran (1995) for an analysis of spread and trading activity changes around earnings announcements.

ent in terms of volatility and volume.[15,16] Again, details are provided in the data section.

B. Matching

Each matched pair consists of a NYSE firm's insider trading day and its counterpart NASDAQ firm's insider trading day that minimizes a metric measuring the difference in match characteristics. The metric is calculated as the sum of absolute valued percentage differences between the NYSE value and NASDAQ value, across four matching variables (described below).[17] We impose one additional criterion: for each matching variable, the paired insider trading days must have characteristic values that are within 20% of each other. This criterion resembles the one imposed by Barber and Lyon (1997) in their matched sample design. They use a 30% cut-off, but find that size matching is poor. Our 20% cut-off appears to create good matches, as we discuss below. The match variables are as follows.

Ex Ante Firm Size. The firm's market value of equity at the end of the calendar year preceding the insider trading date. Numerous papers (see Petersen and Fialkowski (1994), for example) document firm size effects on transaction costs.

Ex Ante Risk. The time-series average of absolute valued daily returns during the window $[t-15, t-11]$ where t is the insider trading day. Higher volatility is typically associated with higher transaction costs (Petersen and Fialkowski (1994)).

Ex Ante Stock Price. The time-series average of daily stock prices during the window $[t-15, t-11]$ where t is the insider trading day. Stock price has a direct effect on proportional effective spreads (see equation (1)).

Ex Ante Typical Insider Trading. Insider trading volume divided by total volume calculated over the calendar year prior to the insider trade year. Firms with substantial insider activity may be recognized and treated differently by market makers than firms with typically minimal insider activity.

III. Data

Insider trading data come from the SEC's Ownership Reporting System. We collect trades by officers and/or directors of the firm during the 1998 calendar year. We do not use trades by principal shareholders (not officers/directors) because Seyhun (1986) shows that these individuals' trades are less likely based on private information.

We use 1998 data because it is the latest full year for which we have insider trading data, and because the SEC's order handling rules were fully implemented

[15]Our conclusions concerning the relative anonymity of the two markets are unaffected by this exclusion.

[16]In addition, Section IV.C presents results from a subset of observations with control days that are very similar to the analyzed insider trading day. Our results hold for this subset.

[17]Matches are not necessarily unique. Two NYSE insider trading days may use the same NASDAQ insider trading day as its peer.

by the end of 1997. These rules improved the execution for NASDAQ trades. There are 102,953 officer/director trades in 1998.

For each insider trading day in a particular stock, we aggregate all trades by officers/directors on that day. Specifically, if the same officer/director trades more than once, or if more than one officer/director trades in the same stock on the same day, we treat all the trades on that day as a single observation. This avoids double counting our microstructure data. We also utilize only those insider trading days that are comprised entirely of stealth (between 500 and 9,999 shares inclusive) insider trades. The result is a sample of 29,656 insider trading days where each officer/director trade on the day was medium- (stealth) sized. Our insider trading data include a cusip number, a transaction date, the insider's position within the firm (officer and/or director), the number of shares traded and whether it was a purchase or sale.

As discussed in Section II, we eliminate all insider trading days with confounding events in the vicinity. Specifically, any observation with earnings or distributions announced and/or paid during the window around the insider trading day or during the control period or in between the two windows is eliminated. In summary, we discard an observation if the confounding event is in the closed interval $[t-2, t+15]$ where t is the insider trading day. This screening leads to 9,302 clean insider trading days on which to collect microstructure data.

Our microstructure information comes from the NYSE's TAQ data. The data we use consist of a bid price, an ask price, a transaction price, and the number of shares traded. Microstructure data is available for 9,045 of our 9,302 clean insider trading days and their associated control days.

Next, we eliminate any insider trading days with control periods that encompass another insider trade (by any officer/director of the firm). This reduces our sample to 7,522 insider trading days.

To further cleanse our sample, we eliminate those observations that apparently had very different control periods relative to the insider trading day. [18] Specifically, any insider trading day/control period pair that has a change in volume or volatility in the top or bottom deciles of their home market sample is eliminated. Large decreases in volume or volatility between the insider trading day and control period suggest that something newsworthy happened on the insider trading day, whereas the reverse, a large increase in volume or volatility between the insider trading day and control period, suggests that the control period encompassed a significant event. This approach has the added advantage of eliminating those cases where our differencing procedure is least likely to control for changes in the fixed costs of market making. Our sample after discarding these observations is 4,899.[19]

We choose the best match of NYSE and NASDAQ insider trading days using the criteria outlined in Section II. We begin with the sample of 4,899 insider trad-

[18]Again, this was noted in Section II. This screening is done after we obtain microstructure data since we use microstructure data to calculate volume (changes).

[19]Also, as noted in Section II, we re-do (see Section IV.C) our analysis for a sub-sample of observations with control days who exhibit very similar characteristics compared to the insider trading day. In particular, we constrain the control day to have values for volume, number of trades, risk, and stock price within 15% of the insider trading day value.

ing days and their associated control periods for which we have microstructure data and that meet the necessary condition for matching. After applying the criteria outlined in Section II, we obtain 335 pairs of NYSE and NASDAQ matched insider trading days (with their associated control periods).[20] This is the sample upon which we perform our main tests.

A. Basic Descriptive Statistics

Table 1 presents descriptive statistics of the daily averages for PES, its time-weighted counterpart (PESTW), average quote midpoint (ABA), total volume of trading (VOLUME), and number of trades (NTRD) on insider trading days. We also report descriptive statistics for the beginning of year (01/01/98) market value of equity (MVEQU), firm Age, and firm Risk for our sample of stocks. We report the numbers separately for NYSE listed and NASDAQ listed stocks. Care should be taken when interpreting these numbers in the context of previous research on spreads, since most measures are calculated on insider trading days. Moreover, comparisons across trading regimes may be misleading at this stage since we have not scaled by the appropriate non-insider trading day values yet. Nevertheless, it is worth noting a few interesting patterns in the data and differences across exchanges.

NASDAQ stocks appear to exhibit higher average and median proportional effective spreads (on insider trading days) than NYSE listed stocks. This finding conforms with more general evidence for that period that spreads are higher on NASDAQ. One potential explanation of higher spreads on NASDAQ (posited by BMW (1992)) is that the superior ability of specialists to identify insider trades will allow them to offer favorable pricing on other trades, leading to lower observed spreads in NYSE stocks on an average day.

Interestingly, there is a real difference between the median volume and number of trades for the matched NYSE and NASDAQ insider trading days. In fact, NASDAQ trading activity appears to be substantially higher, consistent with the "double counting" of trades through a dealer. We therefore feel more comfortable not matching on these two variables.

B. Other Factors that May Affect the Change in Effective Spreads

We also recognize that factors such as risk, stock price, and trading activity can change between the control period and insider trading day, and such changes are likely to affect market maker behavior. Therefore, we investigate whether changes in risk, price, and trading activity are similar for the NYSE and NASDAQ members of the matched pairs. The following variables are investigated.[21]

Relative Price–Ln(Price$_1$*/Price*$_2$*).* Equal to the natural log of the stock price on the insider trading day minus the natural log of the average price during the control period.

[20] If we simply choose the best match without imposing the "within 20% criteria," we reach similar conclusions about relative anonymity in the two markets. However, many of the matches are poor, leading us to focus on the results for the 335 pairs.

[21] We use logs in several cases to reduce the effects of skewness in the variable values.

TABLE 1

Descriptive Statistics

	Mean	Median	Min.	Max.
NYSE ($N = 335$)				
PES (proportional effective spread)	0.0034	0.0025	0.0000	0.0301
PESTW (time-weighted PES)	0.0018	0.0011	0.0000	0.0265
ABA (quote midpoint)	26.42	23.26	2.39	71.20
MVEQU (in $ millions)	1041.99	469.22	28.42	14049.3
VOLUME (no. of shares in 1000s)	65.1	28	1.0	885.7
NTRD (no. of trades per day)	40.69	21	1	393
Age (no. of years listed on CRSP: max =36)	15.3	12	1	36
Risk (avg. $\|\%ret\|[t+11, t+15]$)	0.0163	0.0143	0.0006	0.0504
NASDAQ ($N = 335$)				
PES (proportional effective spread)	0.0048	0.0036[a]	0.0008	0.0249
PESTW (time-weighted PES)	0.0024	0.0018[a]	0.0002	0.0130
ABA (quote midpoint)	26.50	23.67	3.14	73.89
MVEQU (in $ million)	1040.86	451.66	33.32	13980.5
VOLUME (no. of shares in 1000s)	201.2	74.3[a]	2	2257.7
NTRD (no. of trades per day)	154.53	52[a]	1	1731
Age (no. of years listed on CRSP: max =26)	8.89	7[a]	1	26
Risk (Avg. $\|\%Ret\|[t+11, t+15]$)	0.0233	0.0205[a]	0.0011	0.0996

Table 1 presents descriptive statistics of various firm characteristics and market microstructure variables for the stocks in which an insider trades, on the day of that trade (unless otherwise noted). Sample is insider trading days from matched pairs described in Section II. Microstructure numbers are cross-sectional (across observations where each observation is an insider trading day) measures of the mean daily variable (calculated across all transactions in the stock the insider traded) on the insider trading day. Microstructure variables are: Proportional Effective Spread (PES) = [2∗Absolute Value of (Transaction Price−Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2; Total Volume = Volume of shares traded during the day in thousands (this number is not averaged across all transactions on that day—but summed); Number of trades = Total number of trades per day. Firm-specific variables are: MVEQU = beginning of year market value of equity in millions; Age = the number of years the firm has been listed on the CRSP(NASDAQ) tape—max value set equal to 36, age rounded up to nearest year; Risk = the time-series average of absolute valued returns over days $t + 11$ through $t + 15$ (t is insider trading day).

[a]Wilcoxon rank sum test of the null hypothesis that the medians are equal across the two exchanges, reject the null at the 5% level.

Change in Risk—$\{Average|\%Ret|_{[t-2,t+2]}\} - \{Average|\%Ret|_{[t+11,t+15]}\}$. The average over days $[t-2, t+2]$ (t is the insider trading day) of the daily absolute percent return, net of a similarly calculated measure over the control period.

Relative Volume of Trading—$Ln(VOLUME_1/VOLUME_2)$. Equal to the natural log of the total volume of trading on the insider trading day (number of shares traded) less the natural log of the average daily volume (in the same stock) during the control period. Subscript 1 refers to the insider trading day and subscript 2 refers to the control period. Petersen and Fialkowski (1994) document a negative relationship between volume and effective spreads.

Relative Number of Trades—$Ln(NTRD_1/NTRD_2)$. Equal to the natural log of the total number of transactions on the insider trading day (in that stock) less the logarithm of the average number of transactions (in the same stock) during the control period. Jones, Kaul, and Lipson (1994) find that the information in the number of trades subsumes the information conveyed by volume for large firms, while for small firms volume conveys information as well.

Table 2 presents mean and median measures for the above variables, segmented by trading venue. More importantly, it also presents mean and median measures for the differences between the NYSE and NASDAQ pair members' values of these variables. Several items are of note.

First and foremost, the NYSE minus NASDAQ difference in each of the four variables is never significantly different from zero (either in the mean or

TABLE 2

Comparisons of NYSE and NASDAQ Microstructure Responses to Insider Trading when Microstructure Responses are Variable Differentials

(Insider Trading Day minus Non-Insider Trading Day Average Values)

	N = 335					
	NASD		NYSE		NYSE−NASDAQ	
	Mean	Median	Mean	Median	Mean	Median
Change in risk	0.00041	0.00022	0.00117**	0.00136**	0.00076	0.00121
%Δno. of trades	0.11413**	0.14310**	0.14075**	0.18443**	0.02662	0.02537
%Δ in volume	0.14245**	0.13513**	0.14912**	0.14539**	0.00667	0.03390
%Δ in price	−0.00407	−0.00292	0.00495	0.00307	0.00902	0.01158

Table 2 presents descriptive statistics of the differences in key variables between control period days and the insider trading day. Control period is $[t+11, t+15]$ where t is the insider trading day. Sample is insider trading days from matched pairs described in Section II. The tabled variables are: Change in Risk = {Avg. |%Ret|$[t-2, t+2]$} − {Avg. |%Ret|$[t+11, t+15]$}; Percent Change in Number of Trades (%ΔNTRDS) = ln(Total number of trades on insider trading day) minus ln(control period average daily value); Percent Change in Volume (%ΔVOL) = ln(volume of shares traded on insider trading day) minus ln(control period average daily value); Percent Change in Price = ln(average quote on insider trading day) minus ln(control period average daily value). Price is averaged across all trades on a day, while VOL and NTRDS are summed across all trades on a day. Risk is based on close (yesterday) to close (today) returns.

*,** indicate significance with 95%, 99% confidence, respectively.

median). This is important because it suggests that differences in matched pairs' measures of anonymity (changes in effective spreads due to insider trading) are unlikely to be driven by differences in the four tabled variables. Second, the lack of significant NYSE minus NASDAQ differences for the four variables is in spite of individually significant separate NYSE and NASDAQ values. This suggests that our matching algorithm is strong. The fact that individual NYSE and NASDAQ variable values are significant is not an artifact of our sampling method or our matching algorithm. Tests not shown indicate that with or without our screens, insider trading days are accompanied by higher than typical volatility and trading activity.

IV. Anonymity Results

A. Basic Results

To assess the anonymity on the two markets, we compare changes in effective spreads between insider and non-insider trading days for NYSE and NASDAQ firms. Specifically, for each firm/day in the matched pair observation we first calculate the difference between the PES (and PESTW) on the insider trading day and the average PES (PESTW) during the control period. We denote this change in PES as DPES. Next, we calculate the difference between the NYSE and NASDAQ values of DPES (and its time-weighted counterpart DPESTW). Table 3, panel A presents results for DPES while panel B focuses on DPESTW.

In general, specialists on the NYSE appear to react differently to insider trading than NASDAQ dealers. Focusing on the last lines of both panels A and B, the (NYSE−NASDAQ) differences in DPES and DPESTW are significantly positive. Without time weighting, the mean (median) difference in DPES across trading venues is 0.10% (0.039%). Both values are statistically different from zero with 99% confidence. If we time weight our effective spread calculations, the mean (median) difference in DPESTW between NYSE and NASDAQ is 0.055%

TABLE 3

Estimates of Separate NYSE and NASDAQ Responses to Insider Trading (DPES and DPESTW) and Across-Venue Differences: $DPES_{(NY-NQ)}$, $DPESTW_{(NY-NQ)}$

	Mean	Median
Panel A. DPES		
NY DPES	0.00015	0.00000
NQ DPES	−0.00085**	−0.00022**
(NY−NQ) DPES	0.00100**	0.00039**
Panel B. DPESTW		
NY DPESTW	0.00017*	0.00002
NQ DPESTW	−0.00038**	−0.00021**
(NY − NQ) DPESTW	0.00055**	0.00025**

Table 3 presents univariate estimates of the changes in transaction cost due to insider trading and the differences in these values across trading venues. Sample is insider trading days from matched pairs described in Section II ($N = 335$). DPES equals PES on insider trading day minus its average control period counterpart. Proportional Effective Spread (PES) = [2∗Absolute value of (Transaction Price−Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2. PESTW is time-weighted PES.

*,**indicate significance with 95%, 99% confidence, respectively.

(0.025%). Again, both values are significant with 99% confidence. It appears that insiders are less anonymous on the NYSE than on the NASDAQ.

From the first and second lines of each panel, we can draw additional information. The first line of each panel describes the separate NYSE member's (of the matched pair) change in effective spreads due to insider trading. The mean value of DPES (DPESTW) for NYSE insider trading days is 0.015% (0.017%), significant with 90% (95%) confidence. This suggests that insiders appear to be transparent on the NYSE.

The second line from each panel indicates that effective spreads narrow on insider trading days in NASDAQ stocks. It appears that NASDAQ dealers do not adjust to the presence of insiders by raising effective spreads. Smaller spreads could be driven by differences in risk or volume between the insider trading day and control period that we have not adequately accounted for. We revisit this issue in Section IV.C.

Our results also appear to be important economically. The difference of 0.1% between mean NYSE and NASDAQ responses to insider trading (panel A) is 29% (or 40%) of the mean (or median) NYSE effective spread on insider trading days. In dollar terms, the cost of trading 1000 shares of the median stock in our sample (with an approximate price of $23.50) will be $23.50 higher if the stock is NYSE listed.

Overall, given the strength of our matching algorithm, and the insignificant NYSE−NASDAQ differences in Table 2, we conclude that the NYSE is less anonymous than NASDAQ. Nevertheless, we investigate the potential effects of differences in trading activity and match characteristics as an explainer of differential effective spreads in a multivariate framework. The intercepts from these regressions are designed to capture the effect of differential anonymity. We also jointly test whether differences in match variables affect differences in effective spread changes, and separately, whether differences between the insider trading day and control period (in terms of trading activity, risk, and price) affect differences in effective spread changes. Although the results are not tabled, we discuss them below.

Our regression results are generally consistent with the univariate results discussed above. Intercepts from the regressions of NYSE–NASDAQ differences in equal-weighted and time-weighted changes in effective spreads are reasonably close to univariate estimates. Using the equal-weighted proportional effective spread, the intercept of 0.09% is slightly smaller than the univariate estimate of 0.1%. When we use the time-weighted PES, the intercept of 0.1% is somewhat larger than the univariate estimate.[22]

We again document evidence that our matching algorithm works well. In neither regression do we see any evidence that differences in match variables jointly explain significant variation in differential effective spread changes. In the equal-weighted PES regression, we do see evidence that differences between the insider trading day and control period have explanatory power for differential effective spread changes, but not in the time-weighted PES regression.

While these findings provide strong support for the matching algorithm, they raise issues with the use of regression analysis to assess the statistical significance of the intercept. Given the low explanatory power of the regressors (differences between match variables and between insider trading day and control period estimates), the overall explanatory power of the regression is low, biasing down t-statistics. Indeed, the intercept in the equal-weighted PES regression is not significant, while it is only marginally significant (p-value $= 0.071$) in the time-weighted PES regression. We also note that the regression framework imposes strong linearity assumptions that are not necessarily representative of the true trading mechanisms on NYSE and NASDAQ. Finally, any misspecification of the model (not just inappropriately assuming linearity) could add noise and lead to low power. We therefore focus on matched pair differences without regressions in the remainder of the paper.

B. Additional Results

1. Different Matching Restrictions

An alternative to imposing the restriction that matched pair members be within 20% of each other in terms of ex ante variables used to match is to simply select the top quartile in terms of match quality from the full sample of matched pairs. This approach yields a sample of 353 pairs that have the smallest measures for the matching metric. Results for this sample are presented in Table 4. The results are remarkably similar to those found in Table 3. For example, time-weighted measures of relative anonymity (last line of panel B) are 0.055% in the mean and 0.024% in the median—very close to the Table 3 values. Other table values show similar resemblances. Our 20% restriction appears to select a sample representative of the closest possible matches.

[22]The sample used in the equal-weighted PES regression does not include two outliers based on the criteria of Bonferroni. The studentized residuals for these two observations exceed 11 (where four would be the cutoff value given our sample size). The univariate estimate of differential anonymity without these two observations is 0.07%, significant with 99% confidence. The time-weighted PES regression has no such outliers.

TABLE 4

Estimates of Separate NYSE and NASDAQ Responses to Insider Trading (DPES and DPESTW) and Across-Venue Differences: $DPES_{(NY-NQ)}$, $DPESTW_{(NY-NQ)}$

(Lowest Metric Quartile of Matches)

	Mean	Median
Panel A. DPES		
NY DPES	0.00019*	0.00005
NQ DPES	−0.00065**	−0.00022**
(NY−NQ) DPES	0.00084**	0.00038**
Panel B. DPESTW		
NY DPESTW	0.00022**	0.00006**
NQ DPESTW	−0.00033**	−0.00012**
(NY−NQ) DPESTW	0.00055**	0.00024**

Sample is (353) matched pairs with metric in the lowest quartile of the distribution of matched pairs that did not have to meet "within 20%" criteria. Table 4 presents univariate estimates of the changes in transaction cost due to insider trading, and the differences in these values across trading venues. DPES equals PES on insider trading day minus its average control period counterpart. Proportional Effective Spread (PES) = [2∗Absolute value of (Transaction Price − Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2. PESTW is time-weighted PES.

*,**indicate significance with 95%, 99% confidence, respectively.

2. High (NY Firm) Insider Activity Months Sub-Sample

Not all insider trades are necessarily alike. Insiders may be especially active at certain times of the year (for example, shortly after earnings announcements according to Garfinkel (1997)), leading market makers to treat insider trades during such windows differently from trades during less active insider activity windows. We investigate the potential effects of this type of behavior on NYSE−NASDAQ differences in market maker reactions to insider trading in Table 5. Specifically, we focus on the NYSE member of each insider trading day pair, if insider volume relative to total volume in a particular month (of 1998) is in the top quartile of monthly relative insider volumes (using the sample of 335 pairs), then we sample on all insider trades in this month, otherwise we remove the month from our analysis. The number of insider trading day matched pairs that meet this criterion is 84.

Our results are somewhat mixed. While the univariate measures of differential anonymity are all larger than those reported in Table 3, our power is low and not all of the differences are significant. For example, in panel A, the mean and median measures of relative anonymity (NYSE−NASDAQ differences) are 0.117% and 0.073%, while the Table 3 values are smaller. However, neither of the Table 5 values is significant while both Table 3 values are significant.[23] Thus, even though it appears market makers are aware of more pronounced insider activity in some months and are attuned to the potentially higher adverse selection costs associated with this, lack of statistical power prevents us from stronger conclusions.

3. Profitable (NY Firm) Insider Trades Sub-Sample

We also focus on the sub-sample of insider trades in NYSE firm pair members that are profitable. If insiders lose money on a trade (the net of market return

[23]Sampling on insider trading months in the top half of the sample strengthens our results somewhat.

TABLE 5

Estimates of Separate NYSE and NASDAQ Responses to Insider Trading (DPES and DPESTW) and Across-Venue Differences: $\text{DPES}_{(\text{NY}-\text{NQ})}$, $\text{DPESTW}_{(\text{NY}-\text{NQ})}$

(Analysis of Insider Trades from Months with High NY Firm Insider Activity)

	Mean	Median
Panel A. DPES		
NY DPES	0.00003	0.00008
NQ DPES	−0.00114	−0.00055
(NY−NQ) DPES	0.00117	0.00073
Panel B. DPESTW		
NY DPESTW	0.00010	0.00008
NQ DPESTW	−0.00062*	−0.00037*
(NY−NQ) DPESTW	0.00072*	0.00062**

Table 5 presents univariate estimates of the changes in transaction cost due to insider trading and the differences in these values across trading venues. Sample ($N = 84$) is drawn from matched pairs described in Section II ($N = 335$). The drawn observations are those where the NYSE member of the pair had monthly insider volume in the top quartile of all 335 NYSE monthly insider volumes. DPES equals PES on insider trading day minus its average control period counterpart. Proportional Effective Spread (PES) = [2∗Absolute value of (Transaction Price − Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2. PESTW is time-weighted PES.

*,**indicate significance with 95%, 99% confidence, respectively.

over the two years following the transaction is negative), then perhaps they were not trading on inside information. If insiders can credibly communicate this fact ex ante, then market makers may not actively adjust spreads on such trades. This behavior would reduce the probability of documenting significant differences between specialist and dealer responses on insider trading days. We therefore focus (temporarily) on trades more likely based on private information (profitable insider trades). The number of matched pairs meeting this criterion is 179. The results are reported in Table 6.

TABLE 6

Estimates of Separate NYSE and NASDAQ Responses to Insider Trading (DPES and DPESTW) and Across-Venue Differences: $\text{DPES}_{(\text{NY}-\text{NQ})}$, $\text{DPESTW}_{(\text{NY}-\text{NQ})}$

(Analysis of Observations where the NY Firm's Insider Trade is Profitable)

	Mean	Median
Panel A. DPES		
NY DPES	0.00019	0.00004
NQ DPES	−0.00136**	−0.00038**
(NY−NQ) DPES	0.00155**	0.00044**
Panel B. DPESTW		
NY DPESTW	0.00015	0.00002
NQ DPESTW	−0.00073**	−0.00023**
(NY−NQ) DPESTW	0.00088*	0.00028**

Table 6 presents univariate estimates of the changes in transaction cost due to insider trading and the differences in these values across trading venues. Sample ($N = 179$) is insider trading days from matched pairs described in Section II ($N = 335$), where the NYSE member of the pair exhibited positive profit on the insider trade. DPES equals PES on insider trading day minus its average control period counterpart. Proportional Effective Spread (PES) = [2∗Absolute value of (Transaction Price − Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2. PESTW is time-weighted PES.

*,**indicate significance with 95%, 99% confidence, respectively.

Again, our results for a special subset of insider trading day matched pairs may appear to be somewhat stronger than the main sample results in Table 3, but power considerations prevent us from making stronger statements. First, we note statistically meaningful differential anonymity using both equal-weighted and time-weighted measures of changes in effective spreads. However, while the raw numbers suggest stronger effects in this sub-sample than in the main sample, there is no statistical support for a stronger statement. In general, we can simply conclude again that the NYSE appears to be less anonymous than the NASDAQ.

4. Quoted Spread Responses to Insider Trading

Our focus to this point has been upon effective spreads (and their changes) as a market maker response to insider activity. While we believe effective spreads better capture the range of potential market maker activities, for comparison purposes we investigate quoted spread responses to insider trading below. Chung and Charoenwong (1998) investigate the effect of insider trades on spreads quoted by specialists in NYSE and AMEX listed stocks. They do not focus on tests of relative anonymity (comparisons of specialist and dealer reactions to insider trading), while our tests do. Nevertheless, some points of comparison are possible. The results are presented in Table 7.

TABLE 7

Estimates of Separate NYSE and NASDAQ Quoted Spread Responses to Insider Trading and Across-Venue Differences

	Mean	Median
Panel A. DQSPRD		
NY DQSPRD	0.0002	−0.0001
NQ DQSPRD	−0.0012**	−0.0004**
(NY−NQ) DQSPRD	0.0014**	0.0005**
Panel B. DQSPRDTW		
NY DQSPRDTW	0.0003	−0.0001
NQ DQSPRDTW	−0.0011**	−0.0008**
(NY−NQ) DQSPRDTW	0.0013**	0.0007**

Table 7 presents univariate estimates of the changes in transaction cost due to insider trading, and the differences in these values across trading venues. Sample is insider trading days from matched pairs described in Section II ($N = 335$). DQSPRD equals QSPRD on insider trading day minus its average control period counterpart. Quoted Spread (QSPRD) = (Ask price minus Bid price) divided by quote midpoint. QSPRDTW is time-weighted QSPRD.

*,**indicate significance with 95%, 99% confidence, respectively.

In general, we form similar conclusions about relative anonymity when we investigate quoted spreads. If the NYSE is less anonymous than NASDAQ, we would expect more widening of quoted spreads (due to insider trading) from the NYSE members of matched pairs. We find this in both the means and medians.

Comparing our results with those of Chung and Charoenwong is possible if we focus on NYSE insider trading day responses. Similar to their findings, there is no significant evidence of changes in spreads due to insider trading in NYSE stocks. One possible reason for the discrepancy between our quoted spread results and effective spread results is our contention that effective spreads are a better measure of market maker behavior.

C. Magnitude of Inventory and Order Processing Costs

A potential concern with the analysis reported so far is the impact of order processing and inventory costs on the relative spreads. In particular, we have attempted to control for factors other than adverse selection costs by choosing a control period to minimize the impact of order processing and inventory costs. Mathematically, our change in effective spread can be decomposed as follows,

$$\text{(2)} \qquad \text{NQ DPES} = \text{NQ DIOPC} + \text{NQ DASC},$$

$$\text{(3)} \qquad \text{NY DPES} = \text{NY DIOPC} + \text{NY DASC},$$

where DIOPC is the change in inventory and order processing costs and DASC is the change in adverse selection costs. Further, from equations (2) and (3), we see that the difference (NY DPES−NQ DPES) consists of two components,

$$\text{(4)} \qquad \text{NY DPES} - \text{NQ DPES} = (\text{NY DIOPC} - \text{NQ DIOPC}) + (\text{NY DASC} - \text{NQ DASC}).$$

We argue that NY DPES−NQ DPES is a reasonable proxy for NY DASC−NQ DASC, which is only the case if the difference across trading venues in DIOPC is very small and the difference in DASC is relatively large. However, we find that NQ DPES is negative, and a possible reason for this is a poor control for changes in NQ DIOPC. That is, the matching criterion used does not adequately control for order processing and inventory costs, particularly if they are related to risk or trading activity. Therefore, in this section we adopt a more stringent matching criterion that includes risk and trading activity as additional variables along which the firms are matched. This should lead to better controls for order processing and inventory costs.

Specifically, we now choose a single control day from the closed interval $[t-5, t+5]$ (not including t—the insider trading day). Moreover, this day is chosen because it is closest to the insider trading day in terms of volume, number of trades, risk, and stock price. We also ensure there are no other events in this window $[t-5, t+5]$. The key to choosing control days reasonably close to the insider trading days in terms of the four (above) variables is to ensure that there is never more than a 15% (in absolute value) difference between the insider trading day value of this variable and the control day value. Since trading activity is thought of as a key determinant of inventory and order processing costs, this additional restriction gives us confidence that the sub-sample meeting these criteria is more likely to have negligible differences between NY and NQ values of DIOPC.

Given a set of acceptable insider trading day/control day pairs, we then match the NYSE day pairs with the NASDAQ day pairs. Again, we match on the basis of market capitalization, stock price, risk, and past insider trading activity. The final sample that meets all of the above criteria is $N = 26$. The results of our standard tests using this sub-sample are presented in Table 8.

Our conclusions regarding relative anonymity are unchanged. With 95% confidence, we can say that changes in effective spreads are larger on insider trading days in NYSE stocks than in NASDAQ stocks. Focusing on the last line of each panel, both the mean and median differences in effective spreads (either equal- or time-weighted) are positive and significant.

If we examine the individual NYSE and NASDAQ change in effective spread estimates, we see evidence that the above procedures indeed appear to do a better job of controlling for inventory and order processing costs. In only one case

TABLE 8

Estimates of Separate NYSE and NASDAQ Responses to Insider Trading (DPES and DPESTW) and Across-Venue Differences: $DPES_{(NY-NQ)}$, $DPESTW_{(NY-NQ)}$

(Sub-Sample of Very Close Insider Trading Day—Control Day Pairings)

	Mean	Median
Panel A. DPES		
NY DPES	0.00035*	0.00001
NQ DPES	−0.00034	−0.00001
(NY−NQ) DPES	0.00069*	0.00010*
Panel B. DPESTW		
NY DPESTW	0.00018	0.00001
NQ DPESTW	−0.00015	0.00000
(NY−NQ) DPESTW	0.00034*	0.00008*

Table 8 presents univariate estimates of the changes in transaction cost due to insider trading, and the differences in these values across trading venues. Sample is insider trading days from matched pairs chosen while constraining control day to have values of volume, number of trades, risk, and stock price within 15% (absolute value) of insider trading day values. Resulting sample is 26 observations. DPES equals PES on insider trading day minus its average control period counterpart. Proportional Effective Spread (PES) = [2∗Absolute value of (Transaction Price − Prior Quote Midpoint)]/(Standing Quote Midpoint); Quote Midpoint (ABA) = (Ask+Bid)/2. PESTW is time-weighted PES.

*,**indicate significance with 95%, 99% confidence, respectively.

(equally-weighted mean effective spread change), is there marginal (10% level) significance in the NASDAQ decline in effective spread on insider trading days. This contrasts with the mean NYSE effective spread change value and significance level in panel A of Table 8.

Viewing the point estimates from an economic perspective, we see remarkable moves toward zero in the values of the mean and median NASDAQ effective spread changes. Table 3, panel A indicated mean and median values (equally-weighted) of −0.085% and −0.022%, respectively. The time-weighted mean and median were −0.038% and −0.021%, respectively. The corresponding Table 8 values are less than half of these. Controlling for differences in trading activity more carefully appears to render changes in NASDAQ spreads due to insider trading insignificant. It is also worth noting that the NYSE point estimates are either unchanged or more positive.

V. Conclusions

This paper examines the degree of anonymity on different markets. Specifically, we examine the change in spread measures on the NYSE and the NASDAQ on days when an insider trades, to assess the relative ability of specialists and dealers to detect and respond to insider (informed) trading. We find evidence that is consistent with less anonymity in the NYSE specialist system compared to the NASDAQ dealer system. In particular, there is a significant (positive) difference between the average NYSE response to insider trading and the average NASDAQ response. The results support the hypothesis by Benveniste, Marcus, and Wilhelm (1992) that the unique relationship between specialists and floor brokers on the NYSE leads to less anonymity.

Our results are similar if we focus on high (NYSE) insider activity months and profitable NYSE insider trades. They are also robust to concerns about different types of firms listing on different exchanges—we match on key firm characteristics and compare market maker behavior across the matched pairs. We form similar conclusions about relative anonymity whether we investigate changes in quoted spreads or effective spreads, though we believe the latter are more representative of market maker behavior. Finally, our results are robust to a revised experiment utilizing a sub-sample of observations where the insider trading day and control period are much closer in terms of trading activity and risk.

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